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THE ANIMAL AS A MACHINE AND PRIME MOVER.*

THE writer of these papers has been greatly interested in the study of the vital machine in its relations to the special work of the engineer and to the methods illustrated by it in transformation of potential

*Abstracted from *The Animal as a Prime Motor*; N. Y., J. Wiley & Sons, 1894. *Journal of the Franklin Institute*, Jan.-March, 1895.

energies into the mechanical form for useful purposes in the industries.

The value of this form of prime motor to the engineer is enormous, though rarely appreciated or realized. Until the introduction of the steam-engine into mills and factories through the inventions and enterprise of Watt and his partner, at the beginning of the century, horse-power and manual labor only were available for any work for which water-power could not be obtained, and hundreds of horses had even been employed, in earlier times, in draining of single mines. But, even at the present time, the horse is the prime motor for an enormous section of the industries; and all transportation on short routes or available lines, all agricultural work nearly, and work of whatever kind on the highway and in the by-ways must rely on this vital machine for its performance.

The theory of the machine and study of its methods of operation, of energy-conversion, and of economical application of power, is one of the most important subjects practically presented to either the engineer or the man of science, and this for two quite different reasons. In the first place, the vital machine has a higher efficiency than any steam-engine and involves methods of transformation, storage and application of energy which are as yet a mystery, and which, could they be discovered and simulated in engineering practice, might possibly prove

more enormously valuable as improvements upon current methods than was the invention of the modern steam-engine and the displacement of the old machines of Worcester and Savery. It is also possible that nature's ways of producing light and electricity, as well as power, may be ultimately found immensely more economical than those of man. They certainly are quite different, and are inconceivably more efficient in themselves, as single transformations, than any processes yet discovered by science. In the second place, the laws of operation of the vital machine being fully revealed, it is possible that we may find ways of promoting the improvement of the machine in such a manner as to make the animal mechanism a more efficient and a better apparatus for the use of man, and even, perhaps, find ways of improving the instrument employed by the mind in its special operations, as well as the mechanism of the frame in which it is given a home and a vehicle.

The outcome of the investigations made up to the present time may be stated perhaps in the briefest and most intelligible way in the form of a series of theorems, thus:

(1.) *The Vital Machine is not a thermodynamic engine, a heat-motor.*

Many writers have taken for granted the now obviously incorrect hypothesis that, since the machine is evidently a source of heat, and its energy is derived from combustible materials, it must, therefore, be a heat-engine and its operations necessarily thermodynamic. This is easily disproved. In any thermodynamic machine, of whatever class, among the heat-motors, the proportion of heat converted into work, the 'efficiency' of the machine is measured by the range of temperature, from the highest to the lowest in the cycle operated in by the thermodynamic mechanism, divided by the maximum absolute temperature in the cycle. For the

animal machine this would ordinarily be the widest range of temperature attainable in thermodynamic conversion divided by about 300° C. But the machine is, in this case, a mass of circulating fluids of fair conductivity, mainly, and can have no sensible range of temperature, so far as can be seen; and, in fact, it is known that differences of but one or two degrees, in different parts of the body, the only actual differences of temperature, are produced by a slight warming of the venous blood by chemical action, or by proximity to or distance from the epidermis. As a thermodynamic engine, even were it possible, therefore, the machine should have an exceedingly low efficiency. The fact is that its efficiency exceeds that of any heat-engine known to man, under the most favorable possible practical working conditions.

The vital engine is certainly not thermodynamic; its heat is a 'by-product.'

(2.) *The machine is probably not electrodynamic.*

Scoresby and Joule, and Sir William Thomson 'Lord Kelvin' and others among later writers, have suggested that the machine may be, as some have said, an electrodynamic machine, others an electro-magnetic engine. In support of this view it is pointed out that, in some cases, as in the gymnotus, the torpedo and some fifty other creatures, powerful electric batteries, accumulators, are found in the animal system; that all animals seem to have conductors, the nerves, and that electricity *leakage* is always to be detected in the living creature—currents passing in various directions through the body and leaking outward to the surface in all parts. The nerves terminate in 'plates' having close relation in form and structure to the more highly developed cells of the storage batteries of the eel and similar animal producers of electricity.

A great variety of facts and considera-

tions based upon research in this field conspire to indicate, if not to fully prove, that the passage of the electric current along the nerve is the initial act in the motion and energy-production of the muscle. On the other hand, however, it may probably be stated, as conclusively ascertained, that there is no representative of the mechanism of our electro-dynamic machines, either of generator or motor, in the muscle, where, unquestionably, the applied energy is set free and utilized. There is no equivalent of magnet, of solenoid, of field or of armature. On the other hand, it is indicated by numerous and varied investigations and observations that the electric current has for its office, in the vital machine, the promotion of the chemical actions which accompany all motion and development of force and power. The familiar effects of currents having their origin outside the body afford illustrations of the fact and the method of action of these currents. The electric currents, so far as existing in the system, have light work to perform; and where, as in the gymnotus, they are given more formidable tasks, they require for their production and application very large special organs, and occupy an exorbitantly large proportion of the body.

The vital machine is probably not an electro-dynamic motor.

(3.) *The animal prime mover is very probably an example of an exceedingly highly organized and efficient chemico-dynamic motor.*

There are but three known forms of energy available in conversion of the stored potential energy of the foods into dynamic form. Two of these have been seen to be, the one certainly, the other probably, unutilized in the energy-conversion of the vital machine. The third, until some as yet undiscovered process and energy is found to be available, must be assumed to be the source of all dynamic phenomena in the animal system. The machine is probably

a chemico-dynamic prime mover, in which the developments of energy in active form, their magnitude and their applications, are directed by the supreme authority of the system through a very perfect arrangement of electric apparatus, by means of which the necessary orders are telegraphed to the various points at which energy is to be liberated and applied, and by the currents traversing which apparatus the chemical reactions needed in transformation of the potential energy of the fats and glycose, and of the products of broken-down tissue, into active and useful form are inaugurated. Electricity, or some related energy, serves as the directing and stimulating power, and the resolution of fats and other substances into glycosic compounds and their oxidation, at the point at which power is to be developed, into carbon-dioxide and water, by chemical changes resulting in the transformation of potential into actual energies, supplies the working power of the system. The presence of electricity is always observable in the vital machine, and the chemist-physiologists have traced the processes of supply and transportation of potential energy and of the liberation of active energies down to the very last, though still mysterious, act of utilization.

These authorities are now apparently substantially unanimous in declaring it well settled that the action of muscle, for example, is due to what is termed an 'explosive' chemical action in the mass of the organ, the outcome of which is mechanical energy and the liberation of carbon-dioxide. The physicist-physiologists are equally united in testifying that the provocation of this explosive action, at will and in proper quantity, is effected by a nerve-impulse which is more nearly like the electric current than any other known form of physical energy; and the process of doing work by muscular action is likened to the firing of a charge of explosive in the mine by a current

sent over a wire, in this case along a nerve, and the provocation, by its action, of instantaneous oxidation of carbon into carbon-dioxide with change of the physical state from the solid or liquid form into gas liberated in a small space under high compression, and thus in a condition to perform maximum work by its expansion.

(4.) *In this chemico-dynamic machine, the energy displayed in its dynamic operations, as in its muscular work, is generated and applied locally.*

It has been supposed by some writers that the power of the muscular system was derived by transmission from some central or remote source to the point of application, by the nervous system, there to be utilized in the act of muscular stress. It is now well ascertained that not only is there no provision for such transmission of energy, but that the liberation of energy occurs within the mass of the muscle itself, and within its tissue-cells. That the action is local is easily seen in the fact that the excised heart, an excised bit of intestinal muscle, the corpuscles of the blood itself, and the amœbiform protoplasm of which the flesh is composed, in its minutest elements, possess this attribute of energy-development. The heart beats, often for hours in some cases, after removal from the body; the excised muscular tissue exhibits its rhythmic pulsations visibly after isolation; the white blood corpuscle, even, propels itself independently into the locality in which it is to join its energies and activities with those of the already built-up living substance; the elemental protoplasm everywhere exhibits these characteristics of what we call 'living' matter.

Thus complete elemental vital systems are found distributed, in many forms, in all parts of the machine, with their directing and initiative forces as well as their energy-transforming apparatus.

Further: It is now well settled and easily shown that the potential energy supplied

is tendered to the working system in the form of glucosic matter, sugars, produced from fats and starches, and sent through the arterial pipe lines to the capillaries and thence into the very cells of the organs in which work is done. There they are resolved into carbon-dioxide and water; the location and to some extent the nature of the energy-transformation being thus fully revealed. It is a local transformation of chemical into mechanical energy, directly or indirectly, at the very point and in the very cell, apparently, where the work of that elementary portion of potential energy is performed. The question remaining to be solved is whether this transformation is direct or indirect, a single step or a series of energy-changes, not whether it is effected locally or generally or within some special organ appropriated to that duty. Each cell appears to be an elementary prime motor, an elemental vital machine; and the muscular mechanism is a combination of innumerable elements of similar composition and method of action, in each of which a similar process of energy-transformation is conducted.

This process is not thermodynamic, is probably not electro-dynamic, is presumably chemico-dynamic, by which is meant that the energy of chemical action is probably directly transmuted into mechanical energy, not, as in thermodynamic machines, first into heat and then into work. A thermodynamic link in the chain would mean the loss of a large fraction of the whole supply; but it still remains to be ascertained how direct chemico-dynamic conversion of energy can give the remarkable efficiency observed in the vital machine.

(5) *The Nerve-Impulse, the physical energy relied upon for communicating the voluntary and the automatic stimuli which determine the time and intensity of the action of the muscular motor-system, is probably a form of electric energy or some closely related physical action.*

This is a system of telegraphy from nerve-ganglia, spine and brain which does not, as had been formerly supposed by some writers, transmit energy, but simply indicates where and when locally available stored energy is to be liberated and applied to definite purposes by appropriate muscles. It demands energy only in the manner and in the degree in which the electric current firing a mine expends energy in the initiation of the chemical action resulting in the tremendous effects observed. The work is done by the more or less complete transformation of the potential energy available in chemical combinations into mechanical energy, once the electric spark fires the charge.

The passage of the electric current through the fresh muscle produces the same effects as the nerve-impulse, and these effects may be reproduced again and again, until the muscle loses its store of glycose or until its structure changes. At every effort, the flexed muscle consumes glycose and liberates carbon-dioxide, precisely as in its natural operation under the stimulus of the nerve-impulse. This parallelism of action and effect may be taken as, perhaps, good circumstantial evidence. In every animal system, and in every mass of muscle within it, electricity-leakage, or other movements of electricity, may always be detected by the familiar methods of the electrician, and this everywhere distributed energy unquestionably originates in the system itself, and has place and purpose in its economy. In special cases, as in the gymnotus, Nature has magnified its work and given it larger place in the working of the machine than ordinarily, and thus has given us an opportunity to observe, on this magnified scale of working, both the form of the special constructions for the production of this form of energy, and the method of its transmission and application. We find the electric system of the gymnotus to be simply a

development of the nerves and terminal plates found in all animals. That they have a common office, though very different in relative magnitude and importance in the two cases, is undoubted. That the origin, however, of this form of energy, simply as required for telegraphy, is chemical is very certain, also, since it must find its source in the common store of potential energy supplied the whole system. That this chemical process may be somewhat different from that producing chemico-dynamic effects is not improbable; especially as the presence of combustible fats of peculiar composition seem always an essential to nerve action. But all chemical action is accompanied by electric phenomena, and Nature here seems to make the fact subservient to her plans. But she adopts singular methods, and possibly a peculiar form of this energy; and the minute quantity detected by investigators, and the slow rate of progress along the nerve fibers, are elements of as yet unrevealed mystery. The familiar exhausting effect of continued nervous expenditure may be either due to large energy expenditure or to restricted supply of the special form of potential from which it is derived.

(6.) *The nature, source and methods of development and transformation of brain and nerve power do not appear to have been yet discovered, or even surmised.*

The fact that such energy is subject to exhaustion and renewal by precisely the same processes, so far as can be observed, certainly under the same conditions as produce fatigue or favor recuperation of muscular power would seem to justify the inference that the potential energy of the food and the processes of nutrition and of development of active physical energies in the brain, spine and nerves are so modified in these glands as to give a special product in the form of vital energy, and perhaps of brain-power, and of those initiative forces

of the whole nervous system which inaugurate and direct, automatically or intelligently, the currents of nerve-impulse and set in operation and sustain the whole complicated life-system. But how the mind seizes upon these forces and compels these energies to work its will, or how the spine, and automatic mechanism, generally, is set in communication with the mind on the one side, and the organism of the machine on the other, remains a mystery challenging every resource of talent and the highest genius in the investigator. So far as a judgment or even a surmise is permitted, it may probably be assumed that, like all other energies of the vital machine, those of brain and spine and nervous system have a definite, quantivalent relation with the familiar physical energies, and fall within the province of modern scientific research. They demand, beyond a doubt, their proportion of the potential energy supplied in the daily ration.

(7.) *Observed phenomena and statistical data upon which these deductions are founded may be summarized as follows:*

Taking the human vital engine, in illustration, the amount of potential energy supplied the average individual may be taken at 2,500 or 3,000 calories when doing no external muscular work, 4,000 calories when performing a full day's work as a laborer.* This corresponds to 10,000 to 16,000 British thermal units, to from 8,000,000 to 12,500,000, nearly, British dynamic units, foot-pounds per individual per day, of which supply a part is wasted by defective digestion and assimilation and a portion by various defects of the machine itself. Taking the energy-supply of the vital machine as 8,000,000 foot-pounds for the man of sedentary habits and performing brain-work and

10,000,000 for a steady and hard-working laboring man, who does much muscular labor and little thinking, we have the basis of estimates which, though probably not very precise, may yet answer present purposes in giving general conclusions.

Of this eight or ten millions of foot-pounds of energy supplied the machine in potential form, in the foods, not less than fifteen per cent. must be reckoned for deficiency of digestion and transformation into available form in the chyme and chyle, the solutions from which the system draws it for its various special purposes. This seems the minimum usual loss, and an excess is commonly observed, which is furnished by larger food-supply than the assumed figures as here given. A good 'digestion coefficient' is 85 per cent.*

Of the 8,000,000 foot-pounds of energy furnished in the food of the brain-worker, or 10,000,000 supplied the day laborer, something like 7,000,000 in the one case, and 8,500,000 in the other, pass into the reservoirs of potential energy of the vital machine, and circulate in the blood through all its organs; giving up to each that peculiar form of nutriment needed for its work or for its own maintenance. The muscles draw upon it for energy to be converted into the work of external labor or of internal operations essential to life; the various glands elaborate from it those special compositions required for their purposes; the brain and nervous system absorb from it the material for consumption in the operations directed by the mind or automatically conducted by the vital powers of the animal system. Of the 8,500,000 foot-pounds of energy thus furnished the mechanism of the laboring man, in the best cases of application, under most favorable conditions, about 2,000,000 are applied to the performance of

*Pavy on Foods; Mott's Manual; Thurston's Animal as a Prime Mover; Year Book of the New York State Reformatory, 1894; Reports of the Conn. Agricultural Station.

*Flint's Muscular Power; Woods's Digestibility of Feeding Stuffs, Awater's Studies of Diets, Report of Conn. Ag. Experimental Station, 1893.

the day's work ; which is equivalent to saying that the efficiency of this vital machine, considered simply as prime mover, is $23\frac{1}{2}$ per cent. If efficiency of conversion of potential into dynamic energy of muscular work, internal as well as external, is considered, it is very possible that this figure may be doubled, and the efficiency to be taken in comparison with that of heat-engines may be somewhere between forty and fifty per cent. If the internal work of thought and of brain and nerve power is considered useful work, and the total compared with the energy supply, the efficiency will be a still higher figure, perhaps fifty or even sixty per cent.*

But the highest total efficiency of the best steam-engine yet constructed is but about twenty per cent., with its thermodynamic range of about 200° F. (111° C.) degrees, Fahr., and that of the best gas-engine is but about the same, with a range of ten times that extent. If the vital machine be a thermodynamic engine, therefore, its known efficiency, with no recognized temperature, range of heat 'let down,' is not less than twenty-five per cent. higher than, and may be twice as high as, the best heat-engines constructed by man. This is recognized by engineer and thermodynamicist alike, as a *reductio ad absurdum*, and the vital engine is certainly not a heat-engine.

The facts regarding the distribution of potential energy to the various organs of the body; the development by each organ of its special form of product in new compositions or in a special energy; the localization of energy-transformations in the cells of the muscle, or other energy-producer; the accompanying liberation of carbon-dioxide from consumption of glycosic material; the utilization of a telegraphic, or rather a semaphoric, system communication

between the mind or the interior automaton of the spine and cerebellum and the point of useful application of energy all : these are familiar to all physiologists.* Beyond these known phenomena lie the mysteries which the engineers, if possible more than the physiologists themselves, most desire to see completely solved. When they are thoroughly investigated and the operations of the vital machine become fully known, in all their details of energy-transformation, it may be possible to secure new prime movers of similarly high efficiency and thus to double the life of the race by prolonging the period marking the endurance of our supplies of potential energy in the coal-fields of the world. Should it prove that only by preliminary manufacture of fuel, in the form of sugars, can this result be attained, it may seem unlikely that, even when these operations are no longer mysteries, commercial applications of nature's methods can be expected to prove successful; yet when it is considered that the sugars are simply carbon and water, it will not be denied by either engineer, chemist or physiologist that a possibility still remains of effecting so enormously important an advance in the prime motors. If, further, nature's economies in light-production can be paralleled, the engineer may ultimately furnish heat, light and power, the three great products of his special labors of most value to the race, with insignificant wastes and approximately perfect efficiency and maximum cheapness. Given perfect efficiency of power-production and the main problem is solved. R. H. THURSTON.

CORNELL UNIVERSITY.

HARSHBERGER ON THE ORIGIN OF OUR VERNAL FLORA.

By way of a review of a paper by Mr. Henry L. Clarke, in the *American Naturalist*.

* Weisbach's Mechanics of Engineering ; Rankine's Prime Movers ; Thurston's Animal as a Prime Motor ; Reynolds' Memoir of Joule.

* Foster's Physiology ; Encyclopædia Britannica, Art. Physiology ; Chauveau's Le Travail Musculaire.

ist for September, 1893, XXVII., 769-781, entitled 'The Philosophy of Flower Seasons,' I have just contributed an article to the same journal, Feb., 1895, XXIX., 97-117, giving the results of local observations on the same subject. After my paper was in type, I found a short article in *Nature*, XXVII., 7, by J. E. Taylor, entitled 'The Origin of our Vernal Flora,' which suggested some reflections bearing upon the problem. These, with other thoughts relating to the subject, were too late to be incorporated in an article which was already of considerable length. A consideration of these items, however, may not be out of place in connection with an examination of the article by Mr. Harshberger, in *SCIENCE*, Jan. 25, 1895; New Series, I., 92-98.

Commenting upon the fact that it is usual to assign an Arctic origin to our mountain flora, but without giving references, Mr. Taylor says: "Seeing that temperature is so largely influential in explaining the distribution of flowering plants, it occurs to me that not only may height above the sea-level answer to northern distribution, but *seasonal occurrence* as well." Briefly, this covers Mr. Harshberger's propositions numbered 1, 2 and 3 on page 95.

Mr. Taylor observes that the early flowering plants blossom two or three months earlier in Great Britain than within the polar circle. For example, *Chrysosplenium oppositifolium* and *C. alternifolium* bloom 'in March or April; within the Arctic circle not until June and July, and even so late as August.' This suggests a general retardation of flower seasons as we go northward, and I have used this assumption as in part explaining the late blooming of some of the luxuriant, highly specialized groups, which MacMillan* calls 'north-bound.' In many of these, flowering is preceded by a long vegetative period. In the northward movement, if the vegetative period remains

of the same length, it seems probable that the flowering would be later in consequence of this period beginning later.

Mr. Clarke's paper is an elaboration of the idea of the preponderance of the less-specialized flowers in the early part of the season and of the more highly specialized flowers in summer and autumn, and I have criticised this theory from the standpoint of the local flora of my neighborhood, and have undertaken to account for flower seasons as a result of the competition of flowers among themselves and in correlation with the flight of the anthophilous insect fauna. The reader is referred to these papers for a more extended discussion of the relations of flower seasons and the specializations of floral structure.

Mr. Harshberger's observations upon the lull or break in the continuity of the floral procession, which he says at times occurs, is quite interesting. He says: "Such a break seems to occur in the neighborhood of Philadelphia between the twenty-fifth day of May and the tenth or fifteenth day of June, when the first true summer plants appear. Curiously enough, this period corresponds with the time of the ice saints in the United States, when there is a possibility of frost over a large portion of our continental area."

There is a lull, however, which, at least as regards the entomophilous flora, takes place, not 'at times,' but regularly. The frost may, indeed, in many cases have a very definite effect in preventing plants from advancing into the spring months, probably indirectly, however, through its influence upon the vegetative state which precedes flowering. The time of the ice saints, according to Harrington,* is from May 19th to 24th, while the floral depression is later.

In the neighborhood of Carlinville, Ill., the entomophilous flora shows a slight de-

*Higher Seed-Plants of the Minnesota Valley, 1892.

**Harper's Monthly*, LXXXVIII., 878. 1894. Article cited by Harshberger.

cline in June, and many of the groups show well marked June depressions, as will be seen from my curves (in article cited). The dominant families show maxima before June or after, but not one of them shows a June maximum. The depression sometimes occurs in very homogeneous groups, as the Scrophulariaceæ, there being no particular distinctions between the early and late ones. The gap sometimes separates species of the same genus. As a rule, the vernal flowers belong to plants of low habit which bloom in the woodlands, which are now warm and sunny, or upon the open grounds. About June the former become overshadowed by the leaves which have appeared on the trees, and the latter by the later more luxuriant vegetation. Thus the species of *Viola* and *Lithospermum* produce attractive flowers until about this time, when they either stop blooming altogether, or resort to the production of cleistogamic flowers.

One fact, which was not mentioned in my paper, but is shown in my curves, is that the groups of anthophilous insects show the same tendency to form early or late maxima, which emphasizes the importance of the correlations of the two sets of more or less mutually dependent organisms. The Syrphidæ, Empidæ and Andrenidæ show early maxima, while all of the other families show late ones. In the case of the dominant genera of bees, *Anthophora*, *Synhalonia* and *Osmia* reach their maxima early, but the other genera predominate late. *Nomada* breaks into a large early group and a small late one, just like *Andrena*, upon which it is parasitic.

In the case of our trees, I suspect that the flowers were always produced before the appearance of the leaves, an arrangement which would be most favorable to their anemophilous pollination. Mr. Harshberger certainly seems very wide of the mark in explaining the retention by trees of their adaptation for wind aid in transferring their

pollen. In the first place, their height exposes them to the wind in such a way as to make wind pollination quite favorable, while the wind may also interfere with insect visits. The fact that the most highly specialized flower visiting insects are not so abundant in spring will not do, for they are not the insects which are most likely to favor incipient stages of entomophily. The less specialized bees (Andrenidæ) and the flower flies (Syrphidæ) are most abundant in spring, and they would be the most favorable guests in the less specialized states of insect-adaptation. Moreover, flower-loving insects are very abundant in the woodlands in the spring before the leaves appear, and that is the very time that the wind pollinated trees are in bloom. By resorting to entomophily, the trees would only come in competition with the terrestrial flora, which is more favorably situated for insect visits and is very attractive to the early insect fauna.

The author states that "Trees of abnormal habit frequently show atavism, flowering in the late autumn, if exceptionally warm." Such cases as *Hamamelis* are examples. I am inclined to Foerste's* view that the autumnal blooming is a case of precocious development of a spring flower. According to him, *Hamamelis* has distinct hibernacula and in cold autumns holds over until next spring.

If the generally accepted flower theory is true, one would expect to find the highest specialized flowers at that part of the season when the most highly specialized flower visiting insects are most abundant. But it is hard to understand how Mr. Harshberger could attribute this modification to the Lepidoptera. As far as adaptation for flower-pollination is concerned, the bees are beyond question the most highly specialized. Müller† says: "Bees, as the most

* Bot. Gazette, XVII., 3, 1892.

† Fertilization of Flowers, 595, 1883.

skilful and diligent visitors, have played the chief part in the evolution of flowers; we owe to them the most numerous, most varied and most specialized forms." The Lepidoptera have given rise to some highly specialized flowers, but I think it would be hard in a single case to show a probability that the incipient stages of irregularity were induced by their visits.

That the less specialized flowers are spring flowers is only true in a general way. From my present data it appears that the maximum of the entomophilous Choripetalæ is in August, though further observations may show a greater number in spring. Including the anemophilous species, the Choripetalæ will certainly show an early maximum, and that is the extent of the justification of their being called spring plants. The same is true of the entomophilous Monocotyledons. If the blooming seasons of all of the Monocotyledons of a given neighborhood be worked out, I doubt if they will show a vernal maximum, though the position of *Carex* may accomplish this result. The Gamopetalæ have a late maximum, but none of them are free from the competition of the Monocotyledons or the Choripetalæ.

It seems to me that Mr. Harshberger has contributed an important point in reference to the general positions of the flower groups by indicating the influence of the retreat of glacial winter. Making use of this suggestion we may suppose that, as the warm seasons became longer, a large proportion of the Monocotyledons and Choripetalæ moved northward, climbed the mountains or opened their flowers early. While the more highly specialized groups were by no means thus relieved from the competition of the less specialized, there can be little doubt but that in the later months they found a time when that competition was less severe. This may aid us in explaining what has struck me as a fact in the phænological

habits of the flora of my neighborhood. I have indicated that the introduced plants, the aquatics and the degraded entomophilous flowers tend to prolong their blooming seasons, and have supposed that this results from their being more relieved from the competition which besets the other flowers. Although the data have not been arranged to test the point thoroughly, it has occurred to me that the later plants in general bloom longer than the early ones. (In investigating this proposition, it may be proper to eliminate some of the very late ones, whose seasons are not cut short by competition, but by way of preparation for the approaching winter.) The later species thus appear to have entered a position where competition was less severe. It may be, however, that they show the effects of competition less, merely on account of their superiority.

Mr. Harshberger attributes floral modifications to the 'irritating action of insects on vegetal protoplasm.' This suggests Henslow's* theory. As far as I know, that theory has not been accepted by any one who has made a serious investigation of the relations of flowers and insects, and for that reason it has not seemed justifiable to discuss it at length. It seems safe to say that it has not been shown that direct insect contact will induce floral modifications, or that the theory will account for the most ordinary facts of floral structure.

Finally, with regard to the literature, I notice that Mr. Harshberger quotes Mac-Millan (l. c.) without giving references. On consulting this author, I find that the general proposition of the early blooming of the less specialized plants and the late blooming of the more highly specialized is at least strongly suggested, and that too evidently on the authority of persons cited in a bibliographical list. The autumn-flowering of the Compositæ is distinctly stated. From his observations in Flanders,

* The Origin of Floral Structures, 1888.

MacLeod* concludes that the less specialized flowers, as well as insects, prefer the springtime, while the more highly specialized prefer the later months. This anticipates my statement of the same general result.

CHARLES ROBERTSON.

CARLINVILLE, ILLINOIS.

BRISSON'S GENERA OF MAMMALS, 1762.

IN 1756 Brisson published, in Paris, the mammal volume of his '*Regnum Animale in Classes IX Distributum*.' It is a quarto, with the descriptive matter in French and Latin, in parallel columns, and contains a folding table or key on which the generic names are given in proper Latin form. But since the work antedates by two years the 10th edition of Linnæus' *Systema Naturæ*, which by common consent is accepted as the starting point in Zoölogical nomenclature, the names cannot be used. Six years later, however, a second edition of Brisson appeared. It is a rare octavo, wholly in Latin, and was printed at Leyden in 1762†. It is of special importance because it falls between the two editions of Linnæus that are available in Zoölogical nomenclature (10th Ed., 1758; 12th Ed., 1766), and hence may be considered, so far as the genera of mammals are concerned, as a part of the foundation of the nomenclature. The specific names are not exclusively binomial and cannot be used, but the generic names given in the keys (pp. 12-13 and 218) are in due Latin form, and are entitled to recognition.

Although the work was not printed until four years after the 10th edition of Linnæus, the 6th edition (1748) is the only one quoted. Still 25 of the 46 genera given are the same as those published by Linnæus in the

10th Ed. (1758). Of the remaining 21, ten are strictly synonymous with and antedated by Linnæan genera, and consequently cannot be used either in a generic or sub-generic sense. These are:

Brisson, 1762.		Linnæus, 1758.
Pholidotus	=	Manis
Tardigradus	=	Bradypus
Cataphractus	=	Dasypus
Hircus	=	Capra
Aries	=	Ovis
Musaraneus	=	Sorex
Prosimia	=	Lemur
Philander	=	Didelphis
Cetus	=	Physeter
Ceratodon	=	Monodon

The remaining eleven are introduced by Brisson for the first time and are entitled to recognition. They are:

Odobenus	Glis
Giraffa	Pteropus
Tragulus	Hyæna
Hydrochærus	Meles
Tapirus	Lutra
Cuniculus	

Most of these are now in current use, but are attributed to later writers, and in several cases wrong species are taken as types. Carrying the date back to 1762 not only gives them greater stability, but also establishes the types in a satisfactory manner. All but one of the genera take Linnæan species for types, as follows:

The type of *Odobenus* is *O. odobenus* Brisson = *Phoca rosamarus* Linn., which becomes *Odobenus rosamarus* (Linn.) 1758. It thus seems as if the Walrus, after oscillating for a century and a half between *Odobenus* and *Trichechus*, might fairly claim a permanent abiding place.

The type of *Giraffa* is *G. giraffa* Brisson = *Cervus camelopardalis* Linn., which becomes *Giraffa camelopardalis* (Linn.) 1758.

The type of *Tragulus* is *T. indicus* Brisson = *Capra pygmea* Linn., which becomes *Tragulus pygmeus* (Linn.) 1758.

* Over de bevruchting der bloemen in het Kempisch gedeelte van Vlaanderen. Bot. Jaarboek, VI., 1894.

† Regnum Animale in Classes IX. Distributum . . . Quadrupedum & Cetaceorum . . . A. D. Brisson . . . Editio altera auctior . . . Lugduni Batavorum . . . 1762.

The type of *Hydrochæris* is *H. hydrochærus* Brisson=*Sus hydrochæris* Linn. (12th Ed.), which becomes *Hydrochærus hydrochæris* (Linn.) 1766.

The type of *Tapirus* is *T. tapirus* Brisson=*Hippopotamus terrestris* Linn., which becomes *Tapirus terrestris* (Linn.) 1758.

The type of *Cuniculus* may be fixed on *C. cauda longissima* Brisson, which becomes *Cuniculus alactaga* (Olivier) 1800.* *Cuniculus* is one of the few genera in which Brisson did not indicate the type by repeating the generic name for the first species. It was made up of a heterogeneous assemblage comprising no less than six modern genera and five families of Rodents as follows :

<i>Cavia</i> Pallas	1766	(<i>Caviidæ</i>)
<i>Lemmus</i> Link	1795	(<i>Muridæ</i>)
<i>Cælogenus</i> Cuv.	1807	{ (<i>Dasyproctidæ</i>)
<i>Dasyprocta</i> Ill.	1811	
<i>Anisonyx</i> Raf.	1817	(<i>Sciuridæ</i>)
<i>Allactaga</i> Cuv.	1836	(<i>Dipodidæ</i>)

According to the A. O. U. Code, therefore, *Allactaga*, having been left in *Cuniculus* until all the others had been taken out, must stand as the type of *Cuniculus*.

The type of *Glis* is *Glis glis* Brisson=*Sciurus glis* Linn. (12th Ed.), 1766, which becomes *Glis glis* (Linn.) 1766.

The type of *Pteropus* is *P. pteropus* Brisson=*Vespertilio vampyrus* Linn., which becomes *Pteropus vampyrus* (Linn.) 1758, replacing *Pteropus edulis* Auct.†

The type of *Hyæna* is *H. hyæna* Brisson=*Canis hyæna* Linn., which becomes *Hynæa hyæna* (Linn.) 1758.

The type of *Meles* is *M. meles* Brisson=*Ursus meles* Linn., which becomes *Meles meles* (Linn.) 1758.

* *Dipus alactaga* Olivier, Bull. Soc. Philomatique, II., No. 40, 1800, p. 121 ; also Tilloch's Philosophical Mag., Oct., 1800, p. 90.

† See Gray, List of Specimens of Mammals, British Museum, 1843, p. 37 ; and particularly Thomas, Proc. Zool. Soc., London, 1892, p. 316, foot note.

The type of *Lutra* is *L. lutra* Brisson=*Mustela lutra* Linn., which becomes *Lutra lutra* (Linn.) 1758.

C. HART MERRIAM.

NOTES ON AGRICULTURE (I.)

ELECTRO-HORTICULTURE.

THE latest results drawn from experiments with electric light upon vegetation are by Professor Rane in Bulletin No. 37 of the West Virginia Experiment Station. Investigations along this general line began in 1861, when Herve-Mango demonstrated that electric light can cause the formation of green material (chlorophyll) in plants and produce other phenomena, as turning toward the light (heliotropism). Prilleaux, in 1869, showed that assimilation in plants goes on in the presence of artificial light. Dr. Siemens experimented largely with arc lights, both within and at other times outside of and above the plant houses. Professor Bailey, who at Cornell University has tested electric lighting extensively during the past few years, in reviewing Dr. Siemens' work, writes: "He used the term electro-horticulture to designate this new application of electric energy. He anticipated that in the future the horticulturist will have the means of making himself particularly independent of solar light for producing a high quality of fruit at all seasons of the year . . . whatever may be the value of electric light to horticulture, the practical value of Siemens' experiments is still great." After years of trial Professor Bailey stated in one of his reports: "I am convinced that the electric light can be used to advantage in the forcing of some plants."

In the fall of 1892 Professor Rane introduced the use of the incandescent light in place of the arc lamp, and his recent report with its illustrations from photographs of plants, etc., has features of interest to all who are interested in science, as well as the

market gardener. He finds that "the incandescence electric light has a marked effect upon greenhouse plants," it being "beneficial to some plants grown for foliage, such as lettuce. Flowering plants blossomed earlier and continued in bloom longer under the light" than elsewhere. Plants like spinach and endive "quickly ran to seed, which is objectionable in forcing these plants for sale. Most plants tended toward a taller growth under the light." The fact of plants responding promptly to electric light is widely demonstrated, but that it will be an economical method of growing crops is not so clearly shown.

SOIL TREATMENT OF ORCHARDS FOR DROUGHT.

IN many parts of our country crop growing is very uncertain, due to a lack of sufficient rainfall. This fact has led the Nebraska Experiment Station to make a study of methods of mitigating the ill effects of dry weather. Professor Card* reports results upon an old orchard, a third of which was mowed, a third pastured and the remaining third cultivated every two weeks. The trees in cultivated ground suffered much less from the drought and hot winds than those in sod, the foliage being more vigorous and without the wilting during the hot windy days common to the trees in the sod ground.

The fruit was larger and better upon the cultivated trees than elsewhere. An examination of the soil showed that for every 100 barrels of water in the first twenty inches of sod ground there were 140 barrels in the cultivated ground. The soil in all regions when drought is experienced needs a covering of mulch. It is not practicable to add a mulch of straw or other material, but the upper few inches of the soil when kept light and mellow serves as a mulch for all below. Therefore a key to the solution

of the problem is to plow deep; even subsoiling will pay for some crops, and then mulch by means of a mellow layer upon the top produced by frequent cultivation.

THE RUSSIAN THISTLE.

No other species of plant has received so large amount of attention as has been given during the past two years to the Russian Thistle (*Salsola Kali Tragus* (L.) Moq.). Not only the botanists have been interested, but law makers in legislative halls have paused in their party strife to listen to the demands of their constituents for enactments against this newly arrived and miserable plant pest.

Many of the Experiment Stations have published bulletins of greater or less size with full-page engravings of the thistle in its various parts or conditions of growth. Recently a large emergency poster has been issued by a Central-Western Station to be displayed in public places as a means of information and warning to all whom it may concern. The National Government has shared in this work by issuing a bulletin from the Department of Agriculture, while Congress was asked to appropriate vast sums to put down this rapidly spreading, prickly weed.

As the name indicates, this enemy to American agriculture came to our country from Russia, where it is called by a name having the meaning of 'Leap-the-field.' In German it is 'Wind witch,' and with us the same idea is embodied in the name of 'Tumble weed,' namely its capacity for traveling with the wind. When it matures in autumn the stem decays at the surface of the ground, and the large bushy, prickly plant is easily blown for long distances by the wind, and when twenty or so of these plants become entangled and formed into a giant ball the structure is quite formidable.

The new conditions of the far-prairie States, where a rich soil and open country

* 'Some Obstacles to Successful Fruit Growing,' Bulletin 39 Neb. Experimentation Station.

prevail, the spread of this pest has been phenomenal. From a single center in South Dakota, where it was brought in flax seed from Russia a few years ago, it has been disseminated in all directions, so that to-day it may be expected in almost any State in the Union. Its spread is not confined to its natural methods, for with our lines of railway running in all directions the seeds are carried rapidly and for long distances.

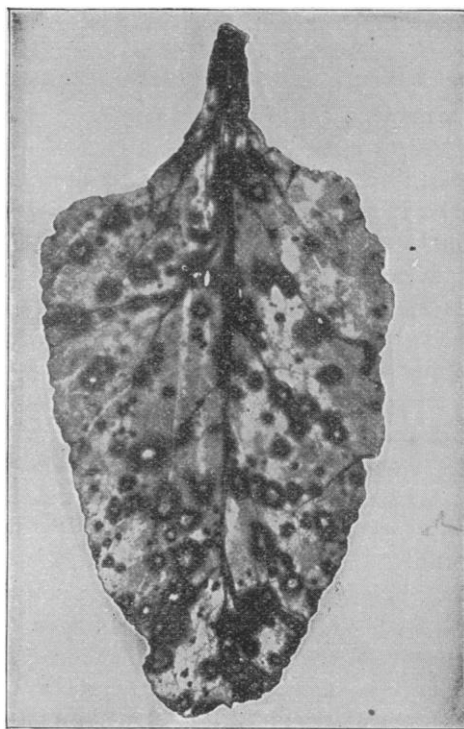
As an outcome of the advent of the Russian thistle, there has been a wide and thorough awakening upon the subject of weeds which will result in a better understanding of these foes, their ways of migration to and throughout our country, and the best methods of subduing them.

THE BEET-LEAF SPOT AND ITS REMEDIES.

THE last Bulletin (No. 107) of the New Jersey Experiment Station describes a fungous trouble of beets in the United States, the *Cercospora beticola*, Sacc., which causes a conspicuous spotting of the foliage. There seems to be no respect shown for any varieties of beets, for the writer has made special visits to the trial grounds of large seed-growers, and all sorts of beets, from the oldest to the newest kinds, were found with their foliage about equally injured.

The common name of 'Leaf Spot' well describes the general appearance of the beet leaves infested with this *Cercospora*, for they are at first more or less covered with small light or ashy spots, which later often become holes by the disappearance of the tissue previously killed by the fungus. Figure 1 is an engraving made from a sun print of a beet leaf, natural size, that was badly infested with the *Cercospora*. Full-sized leaves often become mutilated, and sometimes scarcely more than the framework remains. The fungus itself is quite similar in structure and habits of growth to those causing leaf spots and blights in other

crops. The so-called 'rust' of celery is due to a *Cercospora* (*Cercospora Apii* Fr.), as likewise is the violet leaf spot (*Cercospora Violæ* Sacc.). These fungi consist of slender threads which run through the substance of the leaf, and, coming to the surface in groups, pass through the openings (stomata) in the skin, and in clusters bear long, slender spores in considerable numbers. These spores, when mature, fall from their points of attachment and soon germinate, thus spreading the fungus and causing other spots.



During the past season, under the special charge of Mr. J. A. Kelsey, spraying experiments have been carried out to check the *Cercospora* of the beet. A field of Mangolds, kindly provided by Supt. E. A. Jones, at the College Farm, was experimented upon with Bordeaux mixture.

As the season progressed the Bordeaux

mixture made so striking a difference in the plants that it could have been observed by anyone passing along the side of the field. The untreated rows had the foliage smaller, more upright and badly spotted with the fungus, while the sprayed plants showed a rank growth of foliage, nearly green throughout, more inclined to lop and much less spotted than the untreated plants.

The difference between the roots in the treated and untreated rows shown below in pounds was not so great as that seen in the foliage.

	Sprayed.	Unsprayed.
Roots,	416½ lbs.	331 lbs.
Leaves,	63½ lbs.	49 lbs.
Total,	480 lbs.	380 lbs.

This is an increase of nearly twenty-six per cent., or one-quarter in round numbers. Therefore, the conclusion is that whatever the crop may have been per acre in this case, spraying with Bordeaux mixture would have increased it one-fourth, or, for example, from nine tons to twelve tons.

BYRON D. HALSTED.

SCIENCE IN CANADA.

A NEW volume of the transactions of the Royal Society of Canada (Volume XII.) will shortly be issued. It will be the largest of the series and will contain a bibliography of the work of the Society, collectively and individually. This Society was founded in 1882 by the Marquis of Lorne, at that time Governor-General of Canada, and was organized, to some extent, on the basis of the *Institut de France*. It consists of four sections, of which two are scientific, one being devoted to the physical and chemical, the other to the biological and geological sciences. The system of *éloges*, introduced originally by the French Section (I.), has of late been adopted by the other sections also. Carefully conducted, this feature cannot fail to be of value to the future inquirer. An accurate catalogue of deceased members' works, with their dates of publication, etc.,

and an impartial estimate, ought to accompany the biography.

The scientific members of the Royal Society of Canada comprise several scientific workers and writers of continental, a few of European, fame. Except one year (1891) it has always met at Ottawa, a rendezvous which, though inconvenient for members living at a great distance, has some important advantages, such as access to the National Library, the Archives Bureau, the offices, museum and library of the Geological Survey and the Central Farm, with its laboratories, etc. All these departments are represented in the membership.

Not the least of the services that the Royal Society has rendered to Canada is that which arises from the affiliation of the principal local societies throughout the Dominion. Some of these are important bodies, which publish transactions of their own, and have done a fair share of original work. Among these may be mentioned the Natural History Society of Montreal, founded in 1827; the Canadian Institute (1851), the Hamilton Association (1856), the Nova Scotia Institute of Natural Science (1862), the Entomological Society of Ontario (1863), the Murchison Society, Belleville (1873), the Ottawa Field Naturalists' Club (1879), the Canadian Society of Civil Engineers (1888), the Natural History Society of British Columbia (1889) and the Literary and Scientific Society of Winnipeg (1879). It will be seen that this list practically covers the Dominion from Atlantic to Pacific, and when it is added that every one of these bodies is represented at the May meeting by a delegate, who reads a statement of the year's work, published in the ensuing volume, it will be admitted that the plan is not unfruitful. Some of these allied societies have organized their work into departments, and their reports in the proceedings of the Royal Society form a valuable record of scientific

development. The yearly volume of the R. S. C. is thus both a stimulant and a testimony to scientific progress.

To even outline the character of the work done by the local societies just enumerated would occupy a good deal of space. In some cases the name indicates the general trend of inquiry, but for the most part this can only be learned by consulting reports. The Entomological Society of Canada has long had a reputation for steady and painstaking work, and the commendations that it won at the Centennial Exposition (1876) were not undeserved. The Natural History Society of Montreal has two courses of lectures every winter; the regular monthly meetings yielding papers that are strictly scientific, while the Somerville lectures (founded by a Presbyterian minister more than half a century ago) are of a more popular character.

The two latest of these Somerville lectures were delivered by Prof. Saunders, Superintendent of the Central Farm, Ottawa, and Dr. Robert Bell, F. G. S., of the Geological Survey, their subjects being 'The Resources of the Soil,' and 'The Mammals of Canada,' respectively. Dr. Bell's lecture, which was delivered on the 15th ult., covered an immense habitat or succession of habitats, and was the result of personal observation from the international frontier to the extreme north. The members of the Survey have traversed the vast region between Hudson Bay and the Rocky Mountains, some of them having spent seasons in the Yukon country, others in the Barren Lands. Dr. Bell went on two expeditions to Hudson Bay. In his lecture he spoke of the moose, the red deer, the reindeer, the Rocky Mountain sheep, the antelope, the arctic bear, the seal, the walrus, the whale, the porpoise, the beaver, the cat family, the fox, in his varieties and the smaller mammals, especially the fur-bearing species. He mentioned the domestication of wild animals by the aborigines, and suggested the follow-

ing of their example. The lecture was perhaps rather economic than scientific; though, as largely the result of personal observation, it had a greater value than most popular lectures.

A Montreal society that has been doing good work in an unostentatious way is the Society for the Study of Comparative Psychology, of which Professor T. W. Mills, M. D., author of a work on 'The Dog,' may be said to have been the founder. Most of the papers read at the Society's meetings are based on observations of the habits of animals, several of the members being, like the president, Dr. Mills, connected with the Veterinary College, affiliated to McGill University. At the last meeting (on the 8th ult.), Mr. A. Dell read a paper on the Evolution of Language, Mr. C. A. Bantelle another on Habit. In both observations of animals were used (in part) for illustration. Mr. B. K. Baldwin read a paper on the relation between the intellectual status of the horse and his owner, in which he showed that by sympathy and kindness lower races attained greater control over their horses than higher races without those qualities.

Another society that has been doing some quasi-scientific work is the Folk-Lore, or rather the Montreal Branch of the American Folk-Lore Society. It meets at the houses of members monthly, when papers are read and discussions take place. At the last meeting, Dr. D. S. Kellogg, of Plattsburg, N. Y., gave an interesting paper on the Folk-Lore of the Lake Champlain Valley, the importance of which was increased by the fact that every belief, usage, saying and tradition mentioned had been collected by the essayist in the course of an extensive practice. In almost every case, the source of the story or incident was mentioned. Dr. Kellogg's paper admirably exemplified how profitably a busy professional man, of scientific habit of mind, may utilize his spare *quarts d' heures* and odd moments. J. T. C.

CORRESPONDENCE.

THE CLASSIFICATION OF SKULLS.

EDITOR OF SCIENCE: In 'Varieties of the Human Species, Principles and Method of Classification' (Le Varieta Umane. Principi e methodo di classificazione. Di Giuseppe Sergi. Torino, 1893), which constitutes one of the Smithsonian Miscellaneous Collections, 1894, the skulls are grouped as follows:

NORMA VERTICALIS.

1. Ellipsoid (ellipsoides).
2. Pentagonoid (pentagonoides).
3. Rhomboid (rhomboides).
4. Ovoid (ovoides).
5. Sphenoid (sphenoides).
6. Spheroid (sphaeroides).
7. Byrsoid (byrsoides).
8. Parallelepipedoid (parallelepipedoides.)
9. Cylindroid (cylindroides).
10. Cuboid (cuboides).
11. Trapezoid (trapezoides).
12. Acmonoid (acmonoides).
13. Lophocephalic (lophocephalus).
14. Chomatocephalus (chomatocephalus).
15. Platycephalic (platycephalus).
16. Skopeloid (skopeloides).

In 'Observations upon the Cranial Forms of the American Aborigines based upon Specimens contained in the Collection of the Academy of Natural Sciences of Philadelphia,' by J. Aitken Meigs, Proceedings of the Academy of Natural Sciences of Philadelphia, 1866, 232, occurs the following classification of skulls:

A—Pyramidal or Pyramidocephalic Form.

B—Oval or Oidocephalic Form.

I Cymbecephalic Form.

II Narrow Oval Form (Stenocephalic).

III Broad Oval Form (Eurycephalic).

IV Barrel-shaped or Cylindrical Form (Cylindricephalic).

V Angular Oblong Form.

C—Arched or Hypsecephalic Form.

I Archecephali.

II Phoxocephali.

D—Wedge-shaped or Sphenocephalic Form.

E—Flat or Platycephalic Form (Subglobular).

F—Globular or Sphaerocephalic Form.

G—Square, Cuboidal or Cubicephalic Form.

The two classifications are sufficiently alike to suggest comparisons. Confining my remarks to the forms in Meig's table, which are best illustrated in the *norma verticalis*, I note that:

Oidocephalic = Ovoides.

Cymbecephali = Ellipsoides & Pentagonoides.

Cylindricephali = Cylindroides.

Angularly Oblong Form = Rhomboides.

Archecephali = Trapezoides & Acmonoides.

Phoxocephalic = Lophocephalus.

Sphenocephalic = Sphenoides.

Platycephalic = Platycephalic.

Sphaerocephalic = Sphaeroides.

Cubicephalic = Cuboides.

Thus six out of sixteen names of Sergi's classification are included in Meig's classification. I conclude from comparison of Meig's types with Sergi's figures that the forms are identical.

Ellipsoides and Pentagonoides are included in Cymbecephali; Rhomboides is the same as the skulls included under 'Angularly Oblong Form'; Lophocephalus is a synonym of Phoxocephalic; Parallelepipedoides appears to be a variety of Cylindricephali; Trapezoides and Acmonoides are included in Archecephali.

So long as Sergi endeavors to establish a classification which he desires to be tested by the methods of zoölogy and botany (p. 60), the names he proposes must be judged by the law of priority of publication.

HARRISON ALLEN.

PHILADELPHIA, March 16, 1895.

NOTES ON THE BIOLOGY OF THE LOBSTER ;
A CORRECTION.

IN an article entitled 'Notes on the Biology of the Lobster' (SCIENCE N. S. Vol. I., No. 10, p. 263.) the following sentence occurs: "After hatching a brood in May, the female usually molts and afterwards extrudes a new batch of eggs." This should be corrected to read thus: *After hatching a brood in May, the female usually molts, but does not extrude a new batch of eggs until the following year.*

These notes were culled from a fuller paper, and this slip in the context crept in unobserved. It is, however, corrected in the latter part of the article.

FRANCIS H. HERRICK.

SCIENTIFIC LITERATURE.

THE TYRANNY OF THE MONISTIC CREED, A
REVIEW.

Der Monismus als Band zwischen Religion und Wissenschaft. Glaubensbekenntniss eines Naturforschers. ERNST HAECKEL. Bonn, Emil Strauss. 1893 (Vierte Auflage).

Monism. The Confession of Faith of a Man of Science. ERNST HAECKEL. Translated from the German by J. GILCHRIST. London, Adam and Charles Black. 1894.

The influence of a 'creed' on the progress of science is a proper subject for discussion by men of science, and it is to this, and not to the value of the basis for Haeckel's 'faith,' that we will direct attention.

As he defines it, Monism "is the conviction that there lives one spirit in all things and that the whole cognizable world is constituted, and has been developed, in accordance with one fundamental law."

This positive creed is very different from a modest confession of ignorance, which leaves us free to follow wherever future discoveries may lead, for the monistic creed

is based on the assumption that what we know is a proper measure of what we do not know, as if we could have any measure of the unknown.

An enthusiastic admirer of Haeckel's scientific researches may be pardoned a word of comment on this published statement of his creed.

He tells us all eminent and unprejudiced men of science who have the courage of their opinions think as he does. No one likes to be called a bigot or a coward, or to be accused of ignorance, but those who do not agree with Haeckel must fortify their souls by the thought that this argument is no new thing in history.

Science is justified by works and not by faith, and when Haeckel says 'Credo' and not 'Scio' we need not discuss the value of his belief, although its influence on the progress of science is a more practical matter.

The struggle for intellectual freedom is often called a conflict between religion and science, but while the men of science have burst through those Pillars of Hercules which, according to Bacon, are 'fixed by fate,' they have had no wish to demolish these ancient landmarks, but only to force a passage on to the great ocean of natural knowledge. Least of all do they desire to set up new bounds.

So far a creed involves, or seems to its holders to involve, preconceptions on matters which fall within the province of research or discovery, it is an obstacle to the progress of knowledge and a proper subject for scientific examination.

I shall try to show that the monistic 'confession of faith' has led to the discounting of the possibilities of future discovery, and that it has thus obstructed progress.

One of its results is intolerance of doubt on the problems of life. In this field the monist holds that those who are not with him are against him, and he admits no

middle ground. More freedom is permitted in other fields of thought.

We may say that, since we know nothing about it, we neither believe that the planet Mars is nor that it is not inhabited, but no such philosophic doubt is permitted in biology.

If a teacher of natural science were to say he does not believe life is the outcome of the physical and chemical properties of protoplasm he would most surely be reported as believing it is *not* the result of these properties, and he would straightway be branded a dangerous scientific heretic or a weak brother of the faith, and his confession of ignorance would be put on record as positive belief.

This antipathy to philosophic doubt on the problems of life is clearly due to the dogmatism of the monistic creed, which cannot admit the presence of any unjoined links in our knowledge of nature.

We might be indifferent to this intolerance if it did not cause the most essential characteristics of life to be ignored or pushed into the background.

It is as true now as it was in Bacon's day that: "Whoever, unable to doubt, and eager to affirm, shall establish principles proved, as he believes, . . . and according to the unmoved truth of these, shall reject or receive others, . . . he shall exchange things for words, reason for insanity, the world for a fable, and shall be unable to interpret."

The essential characteristic of life is fitness.

A living organism is a being which *uses* the world around it for its own good.

I, for one, am unable to find, in inorganic matter, any germ of this wonderful attribute.

It is possible that after chemistry has given us artificial protoplasm this may be shaped, by selection or some other agency, into persistent adjustment to the shifting

world around it, and that it may thus become alive.

Everything is possible in the unknown, but why should we believe anything on the subject until we have evidence?

Of one thing we may be sure. The artificial production of protoplasm would not be a solution of the problem of life. The nature of the problem must be grasped in all its length and breadth, with all its difficulties, before we can hope to solve it.

Many biologists have sought to solve it by transforming Huxley's carefully guarded statement that protoplasm is the physical basis of life into the dogma that life is the sum of the physical properties of protoplasm.

Life cannot go on without food, and we may say with propriety that bread is the *staff* of life, but the agency which shapes the food into the specific structure of an organism exquisitely adapted to the conditions of the world around it is to be sought somewhere else than in the properties of bread.

One of the distinctive characteristics of this organizing agency is that it may exist in a germ without any visible organization. Another is that, so far as we know, it has been handed down, in an unbroken line, from the oldest living things, generation after generation, to the modern forms of life, and that it has leavened the whole hump of living matter.

While we know nothing of its nature or origin, and must guard against any unproved assumption, there seem, from our present standpoint, to be insuperable objections to the view that this agency is either matter or energy. While we know it only in union with protoplasm, it would seem that, if it is matter, it must, long ago, have reached the *minimum divisibile*. If it is energy, or wave motion, or perigenesis of plastidules, it is hard to understand why it has not been dissipated and exhausted. We know that it exists, and this is in itself a fact of the utmost moment.

We are told that the belief that it has, at some time, arisen from the properties of inorganic matter is a logical necessity, but the only logical necessity is that when our knowledge ends we should confess ignorance.

Young men who have been trained in the routine of the laboratory tell us all their interest in biology would be gone if they did not believe all its problems are, in the long run, to be resolved into physics and chemistry.

The only answer we can give them is that noble work has been done in natural science by men like Wallace, who believe that life is fundamentally different from matter, and also by men like Haeckel, who believe the opposite.

They also serve science who only stand and wait, and among them I would wish to be numbered.

While nothing is gained by giving a name to the unknown agency which is the essence of life, it is better to call it a 'vital principle' than to deny or ignore its existence. It is better to be called a 'vitalist,' or any other hard name by zealous monists, than to be convicted of teaching, as proved, what we know is not proven.

The word *vitality* is as innocent as *electricity* or *gravity*; in fact, Newton's use of this word led Leibnitz to charge him with infidelity to the spirit of science, although no one need fear to follow where Newton leads.

The older vitalists may have looked on a mere word as an explanation, but the reason the word has fallen into disrepute is the antagonism of the monists to the view that the problem of life presents any peculiar difficulties.

Many thoughtful men of science have held that the 'faith' of men like Haeckel ignores many of the data which are furnished by our scientific knowledge of the world around us.

Huxley, in his essay on the Physical

Basis of Life (1868), says it is necessary for a wise life to be fully possessed of two beliefs: "The first, that the order of nature is ascertainable by our faculties to an extent which is practically unlimited; the second, that our volition *counts for something as a condition of the course of events*. Each of these beliefs can be verified experimentally as often as we like to try."

Again, twenty-five years later (1893), he says (*Evolution and Ethics*) that, fragile reed as man may be, "there lies within him a fund of energy, operating intelligently, and *so far akin to that which pervades the universe that it is competent to influence and modify the cosmic process*."

Clearly this man of science has no overwhelming dread of the charge of anthropomorphism or animism, or of any charge except lack of caution.

I think that he would also admit that every living thing contains some small part of this influence which 'counts for something as a condition of the course of events,' and that it must be reckoned with in our attempts at a philosophy of the universe.

W. K. BROOKS.

JOHNS HOPKINS UNIVERSITY.

The Life and Writings of Constantine Samuel Rafinesque. (Filson Club Publications No. 10.) Prepared for the Filson Club and read at its meeting, Monday, April 2, 1894. By RICHARD ELLSWORTH CALL, M. A., M. Sc., M. D. Louisville, Ky., John P. Morton & Co. 1895. 4to. pp. xiii + 227. Portraits, etc. Paper. Price \$2.50, net.

This sumptuous volume is published by a Historical Club in Louisville, Kentucky, as a memorial to one of the pioneer naturalists and explorers of the Ohio valley, a man whose brilliant intellect, eccentric character and unhappy fate will always cause his career to be looked upon with interest, and whose nervous and appalling industry has

been the cause of a myriad of perplexities to students of the nomenclature of plants and animals in Europe as well as in America.

Born in Constantinople in 1783, his father a French merchant from Marseilles, his mother a Greek woman of Saxon parentage, Constantine Rafinesque early entered upon the career of a wanderer. The roving habit of mind which soon became a part of his nature led him into a mental vagabondage that influenced his career even more than the lack of a permanent place of abode. His youth was passed in Turkey, Leghorn, Marseilles, Pisa and Genoa. He had good opportunities for study and reading, and before he was twelve had, as he himself records, read the great Universal History and one thousand volumes of books on many pleasing and interesting subjects. He was ravenous for facts, which he gathered, classified and wrote down in his notebooks. He began to collect fishes and birds, shells and crabs, plants and minerals, found or made names for them, copied maps from rare works, and made new ones from his own surveys. His precocious mind, unguided and undisciplined, wandered at will over the entire field of books and nature, and by the time he reached the age of nineteen he had formed his own character and equipped himself for the career which lay before him. He became a man of catalogues, of categories, of classifications. He possessed much native critical acumen, and it is possible, though scarcely probable, that as his present biographer suggests, had he during the formative period been firmly guided by some master hand, he might have become one of the world's greatest naturalists. Lacking such guidance, however, he was by no means fitted to enter upon a scientific career in a country like the United States, so when, at the age of twenty, he crossed the Atlantic he brought with him the germs of failure and bitter disappointment.

From 1802 to 1805 he lived in Philadelphia. From 1806 to 1815 he was in Sicily, where he did some of his best work in his 'Index to Sicilian Ichthyology,' and in his often quoted 'Caratteri.' Here he established his monthly journal, the 'Mirror of the Sciences' (*Specchio delle Scienze*, etc.); which endured throughout the twelve months of 1814, but ended with its second volume. Rafinesque was not only the editor, but almost the sole contributor to this journal, in which he printed no less than sixty-eight articles upon a great variety of subjects—upon animals, plants, minerals, meteorology, physics, chemistry, political economy, archæology, history and literature, besides many critical reviews. His fatal tendency to 'scatter' was already apparent, and in the work which he did for the 'Specchio' all the weaknesses of his subsequent career were foreshadowed. While in Sicily, for political reasons, he assumed the surname, Schmaltz, that of his mother's family.

In 1815 he returned to America, and was shipwrecked on the coast of Connecticut, losing all his books, manuscripts and collections. For the next three years he lived in New York, and during this period he contributed to the 'American Monthly Magazine' a number of really brilliant and learned articles. So masterly, indeed, were these that it seemed as if he were likely to become one of the leaders in American scientific thought. It seems probable that he was at this time steadied and guided by his friend and patron, Dr. Samuel Latham Mitchill, whom he greatly respected and admired; at all events, when he left New York, signs of deterioration appeared in his methods. In 1818 he crossed the Alleghanies, and in the following year became a professor in the Transylvania University, at Lexington, Ky.

There he remained for seven years, sadly ill at ease among the old-school college professors who composed the faculty, yet, from

the showing even of his own complaints, treated with singular indulgence by them, and allowed to devote the most of his time to his excursions and to his writing. While here he printed nearly one hundred papers, chiefly descriptions of new plants and animals. From 1825 to 1840 his life was so irregular and his wanderings so extensive that his biographer has made no attempt to follow its course. Philadelphia was his home, when he had one, but he was a soured and disappointed man. His health was bad, and he could not get any one to print his voluminous writings. He established his 'Atlantic Journal,' which soon failed. He published various works by subscription, and also added to his income by the sale of 'Pulmel,' a medicine for the cure of consumption, concerning which he wrote a book. In his later years he established in Philadelphia his 'Divital Institution and Six Per Cent. Savings Bank,' which seems to have had some degree of success. He died in 1840, in poverty and almost friendlessness, and is buried in an unmarked grave.

His career is described well and in sympathetic mood by Professor Call, who sums up the story of his last years in these words: "The experiences through which he had passed, which involved some of the saddest that come to men, had so broken him that there is little question that he was not of sound mind during these latest years. He was not, however, the irresponsible madman some would have us believe; rather, his was monomania and took the direction of descriptions of new forms of plant and animal life. But more than this, his defect was that peculiar form of monomania which believed only in himself; which gave his own work a value which does not always attach to it; which made him neglect the work of others, or, if it were noticed, impelled him to caustic and unwise criticism."

This judicious estimate, which is intended by Professor Call to apply only to his

later years, I should be disposed with some slight reserves, to accept as a fair summary of his entire life-work, for all of the faults of his latest works were, as I have already suggested, foreshadowed in his Sicilian writings of 1814. The sympathy which I once felt for Rafinesque has almost vanished with the reading of the whole story of his life, for the man, as shown by his own private papers, appears to have been singularly unsympathetic and unlovable, enveloped in a mantle of self-esteem and interested in natural objects solely because he found in them something to name and to classify. In all his writings there appears scarcely a gleam of love or enthusiasm for nature, and he speaks of his fellow-men only in words of criticism or malediction. It would doubtless have been much better if he had never touched pen to paper. The fact that he had a keen eye and a remarkable power of diagnosis, and that he had learned the methods of systematic description, made his activity all the more pernicious, since regard for painstaking accuracy was as foreign to him as love of nature.

The canons of nomenclature which now prevail among American naturalists force them to take cognizance of all his descriptions and to use his names, whenever by any possibility his meaning can be determined. In many instances I have known him to be given the benefit of a doubt. So the unwelcome name of Rafinesque is constantly obtruding itself in almost every branch of zoölogy and botany, and it is likely to remain for a long time an obstacle in the way of securing the recognition of American nomenclature in Europe. He stands nevertheless as an important figure in early American biological literature, and whether we like him or not he cannot be ignored. It is fortunate, then, that all relating to his work has at last been brought together in so convenient a form.

The minute and scholarly bibliography,

which includes in all 420 titles, is most valuable. Professor Call's estimate of the value of these writings is a very kindly one. Bad as it was, Rafinesque's work unquestionably entitles him to recognition as the pioneer student of the ichthyology and conchology of the Mississippi valley, and he was also among the earliest to study its botany and its prehistoric archæology.

All the existing portraits of Rafinesque are reproduced, as well as a specimen of his handwriting, and in the appendix is reprinted his will, which affords a better insight into his character than all else he ever wrote.

The book is exhaustively complete, well written and beautifully printed, and in its publication the author and the Filson Club have accomplished admirably the task which they had undertaken. They have reared a noble monument to him who was 'the first Professor of Natural Science west of the Alleghanies.'

G. BROWN GOODE.

The Royal Natural History. Edited by RICHARD LYDEKKER. Illustrated by 72 colored plates and 1600 engravings. Frederick Warne & Co., London and New York. Royal 8°. 1894-95. Issued in monthly parts.

The second full volume of this important work is now out and, like the first, is devoted entirely to the Mammalia. The first comprised the Apes, Monkeys, Bats, Insectivores and part of the Carnivores; the second completes the Carnivores and includes also the Ungulates, Manatees and Dugongs. The well-known reputation of the editor and principal author, Mr. Lydekker, gives special value to these parts.

In general scope and plan of treatment the work resembles Brehm's *Thierleben*, of which several editions have appeared in Germany, and the *Standard Natural History*, published in this country. The illustrations

are in the main borrowed from Brehm; they were pirated by the *Standard Natural History* ten years ago, and here appear for the third time. Of course this is not the fault of the author; but it is a pity original works cannot have original illustrations. Good plates are as much a part of a book as the text itself, and should be allowed to stand unmolested as monuments to the author. It is not intended to deprecate the exchange of technical figures or the judicious bringing together of scattered cuts illustrating special subjects—a very different thing from the wholesale reproduction of a previous author's pictures.

The original cuts are not of high merit. Those of the hooded seal and skull of the cave bear are gross caricatures, and nearly all the skulls and teeth are far inferior to modern standards for such work; and it is not too much to say that Mr. Lydekker himself, in previous publications, has done much toward fixing these higher standards. The colored plates are cheap chromos, in striking contrast to the excellent and artistic plates borrowed from Brehm.

In quoting American writers on 'big game' the most authentic and best informed writers are not always chosen. The one book that is beyond all comparison the best yet written on our larger mammals—I refer of course to Roosevelt's *Wilderness Hunter*—is apparently unknown to the editor. As a natural result some surprising statements are made, as, for instance, when Oregon antelope hunters are told that the pronghorn has 'almost or quite disappeared' from their State.

Some confusion arises from different usages of the common names of animals. The statement that in North America "the range of the *elk* appears to have extended originally from about the 43d to the 70th parallel of latitude, its northern limit being marked by the southern limit of the so-called barren grounds," will take the breath

away from most Americans who read it for the first time, but a careful perusal of the context shows that our *moose* is the animal meant.

The hooded seal is said to be 'nowhere met with in large numbers,' a statement that will bear qualification in view of the fact that many thousands are sometimes taken by single vessels at the Newfoundland and Labrador seal fisheries. More than 15,000 were killed on the ice and brought to Newfoundland in March, 1883, by a sealer—the *Proteus*—which I accompanied as surgeon-naturalist, and similar catches are not rare.

In the matter of genera, the comprehensive groups of the past are commonly used instead of the smaller groups of to-day. The same conservatism characterizes the treatment of species—perhaps a good fault in a popular work, though one that can be carried too far—as when a dozen skunks are lumped under a single name, and the most specialized of our true foxes is left out.

The author seems to be constitutionally averse to the recognition of American species as distinct from their European representatives. This is shown by his treatment of our wolf, red fox, lynx, wolverine, marten and weasels. Even in the case of the mink the opinion is expressed that the American and European animals are 'mere local varieties of a single species.' The only explanation of such statements, from a man of Lydekker's experience in studying fossil mammals, is that he has not personally compared the skulls and teeth of the American and European forms. The number of American species is reduced out of all proportion to the sharpness of their characters or the size of the areas they inhabit. Thus, while three martens are accorded specific rank for Eurasia, only one is allowed for America, and it is given as doubtfully distinct. It should be stated, however, that no European collection of mammals con-

tains more than a fraction of our species; hence it is not so surprising that a foreign author should fail to appreciate their characters.

The common skunk of New England is said to range from Hudson Bay to Guatemala, but it does not reach even the Southern United States. Again, skunks are said to be good climbers, but neither *Mephitis* nor *Conepatus* can climb trees—the ability to do this being limited to the agile weasel-like members of the genus *Spilogale*.

The article on the fur seal is full of misstatements and savors too strongly of a political argument from the British side of the case. The number of fur-seals killed at the Pribilof Islands each year is said to be 'limited to 100,000,' and it is implied that the number actually killed is still larger. As a matter of fact, 100,000 have not been killed since 1889, while the number killed at the islands since 1890 is as follows: 1890, 25,701; 1891, 14,406; 1892, 7,509; 1893, 7,390; 1894, 15,033.

We are told that the seals taken at sea (by pelagic sealers) 'appear to be exclusively young males or barren females.' In reality the great majority of these seals are breeding females. The author's ideas of humanity are simply past comprehension. He says: "Of the two methods of sealing, the shooting in the open sea is decidedly to be preferred on humanitarian grounds, more especially if it be true, as asserted, that on the Pribiloffs a considerable number of breeding female seals are killed before their cubs are old enough to shift for themselves." No female seals are ever killed on the islands except by accident—possibly one in many thousands—while in the open sea, as already stated, the great majority are females. Of these females, those killed on their way to the islands in spring are heavy with young, and those killed in Bering Sea in summer are nursing; so two lives are sacrificed for every one taken. Ever since

pelagic sealing has been carried on in Bering Sea, thousands of motherless 'pups' have died on the islands each year of starvation.

It is lamentable that the author has been so grossly deceived in these matters, and still more unfortunate that a scientific work should be tainted with partisan odor.

It is stated that no islands in Bering Sea besides St. Paul and St. George are inhabited by fur-seals. This must be a slip of the pen, for of course Mr. Lydekker knows that the Commander Islands are the breeding grounds of the *west* Bering Sea herd, just as the Pribilof Islands are the home of the *east* Bering sea herd.

In the matter of nomenclature the author seems to be on the fence. In some cases the law of priority is rigidly enforced; in others a name in common use is retained rather than the earlier name. Preoccupied generic names are as a rule discarded, but *Bassaris*, though preoccupied, is given instead of *Bassariscus*—doubtless by oversight.

The author's attitude as to genera is shown by the remark that in a certain group only one genus can be admitted 'on account of intermediate forms.' Is this not a surprising position for one of the most distinguished of living paleontologists? Are not all mammals connected by intermediate forms, living or extinct, even if all are not yet discovered? And would not Lydekker's system, if logically enforced, result sooner or later in the destruction of most of our generic groups? Is it not more rational to found genera on the weight of characters as presented in extremes of differentiation rather than on the accident of the survival or extinction of annectant species?

As a general criticism of the Royal Natural History, so far as now issued, it may be said that the parts on American mammals are weak. On the other hand, the foreign species—foreign from our stand-

point—are treated with a fullness and reliability not to be found in any other work. The magnitude of the undertaking and the haste in which the parts had to be prepared (to appear monthly) inevitably led to occasional inaccuracies; but the defects are far outweighed by the merits, and the work will prove helpful to naturalists and amateurs alike for many years to come. It is, indeed, a great satisfaction to be able to turn to a single publication in which the principal facts respecting the mammals of the world are brought down to date and stated with clearness and authority.

C. HART MERRIAM.

The Book of Antelopes. By P. L. SCLATER and OLDFIELD THOMAS. Illustrated by JOSEPH WOLF and J. SMITH. 4°, London, R. H. Porter, 1894-95.

The second part of this handsome and useful work, dated January, 1895, has come to hand. The distinguished authors make no attempt to offer a complete scientific treatise on the antelopes, but furnish "descriptive letter-press [with full synonymy] for the beautiful series of lithographic plates drawn some twenty years ago under the supervision of the late Sir Victor Brooke, making thereto such necessary modifications and additions as the progress of science demands."

The work comprises the diverse members of the Bovidae commonly called antelopes, hartbeests, gnus, duikers, water-boks and gazelles, and also the gemsbok, saiga, oryx, eland and many others. The geographic range of each species is given, together with an interesting account of its habits and peculiarities. Besides the full page colored plates, there are many excellent cuts in the text, mostly of horns and skulls. The book therefore is helpful alike to the naturalist and the sportsman, and is a handsome addition to any library.

The animals treated in the first two parts

are the hartbeests and gnus (genera *Bubalis*, *Damaliscus* and *Connochætes*), all belonging to the subfamily Bubalidinae, and residents of Africa and Arabia. Twelve colored plates have been issued, and seven are promised with the next number, which will be devoted to the duikers (*Cephalophus*).

C. H. M.

NOTES AND NEWS.

PROF. S. CALVIN, State Geologist of Iowa, announces that reprints of the photographs accumulated by the survey may be had for 12½ cents each. A descriptive list of views may be had on application to the State Geologist at Des Moines; all orders to be made by the numbers of this list. If this practice were generally adopted by our State Surveys, it would be greatly to the advantage of many students and teachers.

PROF. W. R. NEWBOLD, of the University of Pennsylvania, has become one of the associate editors of the *American Naturalist*. In the current number he gives an account of 'The Present State of Psychology.'

DR. WIRTINGER has been made Professor of Mathematics in the University of Innsbruck.

DR. WILDER D. BANCROFT, now Instructor in Harvard University, has been appointed Assistant Professor of Physical Chemistry in Cornell University.

PROF. FRANCIS GOTCH, now of University College, Liverpool, has been elected to the Waynflete Chair of Physiology at the University of Oxford, vacant by the transferrence of Prof. Burdon Saunderson to the Regius Professorship of Medicine.

JAMES E. OLIVER, Professor of Mathematics in Cornell University, died at Ithaca, on March 28th.

THE DUKE D'ORLÉANS has presented the Imperial Institute of London with his extensive collection of specimens of natural history, costumes and curiosities.

MR. LESTER F. WARD writes that he has just received from the family confirmation of the reported death of the Marquis Saporita. He died at Aix on January 25th.

DR. NIKOLA TESLA suffered a serious loss in the destruction of his laboratory by fire on March 13th.

GENERAL DE MASOUTY, Founder and Director of the Pic du Midi Observatory, died recently at the age of eighty years.

DR. HARRISON ALLEN has been elected a member of the Council of the Philadelphia Academy of Natural Sciences, to fill the vacancy caused by the death of John H. Redfield.

THE Joint Commission of the Scientific Societies of Washington has begun to publish a monthly programme, giving the dates of meeting of the various scientific societies of Washington for the ensuing month and a full list of papers to be presented.

AT a meeting of the New York Alumni of the Johns Hopkins University, on March 29th, President Gilman made an address on 'Impending Problems in Education.' An address was also made by Professor Butler, of Columbia College.

A NATIONAL Exhibition of Industry and Fine Arts in the City of Mexico will be opened on April 2d, and an International Exposition is proposed for Montreal, to be opened on May 4th.

AT the last meeting of the Academy of Natural Sciences at Philadelphia, Miss Emma Walter read a paper entitled *Does the Delaware Water Gap Consist of Two River Gorges?* She adduced evidence to show that the river once flowed through the Gap from the south towards the north; that this north-flowing river was pre-glacial, and that much the greater part of the erosion is the work of this old river, the remainder being due to the action of the present south-flowing stream.

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES.

THE Section of Geology and Mineralogy met March 18, and after electing as officers for the ensuing year, Prof. J. J. Stevenson, chairman, and Prof. J. F. Kemp, secretary, listened to a lecture by Prof. J. J. Stevenson on 'The Origin of the Pennsylvania Anthracite,' of which the following is an abstract:

Long ago H. D. Rogers showed that the coal regions of Pennsylvania are divided into longitudinal basins or troughs. The first series embraces the area between the Great Valley and the Alleghany Mountains and contains the several anthracite fields as well as the semi-bituminous fields of Broad Top and the Potomac River. Beyond the Alleghanies are six well marked basins containing bituminous coal.

Along a line from central Ohio, eastward to the Potomac coal field, one finds noteworthy variations in dip, the amount being insignificant in Ohio, but very great in the first series of basins. The increase is not regular, there being no change practically from the coke basins of eastern Pennsylvania until within three or four miles of the Potomac field, where the dip becomes very abrupt. This line shows the extremes of variations, for further northward there is in all of the basins a diminution of disturbance, even in the anthracite areas, while southward there is a similar decrease, except in the last.

Analysis of coal samples from the Pittsburgh bed, in the several basins, show a progressive decrease in proportion of volatile matter toward the east or southeast. H. D. Rogers regarded this decrease as due to influence of steam or other gas escaping from crevices made during the folding of the rocks, for he asserted that the volatile increased as the flexures diminished in strength. Stevenson in 1877 showed that no such relation exists. Lesley in 1879 thought that earth-

heat might have caused the change, as coals in the anthracite region were buried under a very deep covering of rocks; but there is no evidence that the coal measures were thicker at the east than in western Pennsylvania, while there is every reason for supposing that the coal measures were thinner there than at the southwest. There is therefore no good ground for supposing that the earth-heat would be effective, for in Virginia, where the thickness is very great, the coals at the bottom of the column are very rich in volatile matter.

Professor Lesley has suggested that the change in the coal might have been due to oxidation. The rocks of the anthracite region are consolidated gravels with little of argillaceous matters, whereas those of the bituminous area are largely argillaceous, which, being undisturbed, lute down the coals, preventing percolation of water and the escape of gases. But in fact the bituminous fields afford all types of coal from highly bituminous to hard anthracite, and sections in many portions of the anthracite fields show more clay beds than do those in S. W. Virginia where the coal is highly bituminous.

It is not necessary to regard metamorphism as the sole cause of anthracite. It is not called in to explain a variation of ten per cent. in the same beds within short distances, and it cannot explain the occurrence of bituminous in one bench and of anthracite in another in the same opening in Sullivan County, Pa., or equally of semi-bituminous and dry anthracite in different benches of the Mammoth. It does seem as though the conversion of the coal must have been practically complete before entombment; otherwise the variations of coal of the same age in different areas would seem to be inexplicable.

In Pennsylvania the decrease in volatile bears no relation to the extent of plication, but it bears close relation to the thickening

of the coal. The decrease in all of the areas is toward the old shore line at the north and northeast. In the anthracite area it is very gradual until one passes the prongs in the southern field, where the thickness of coal increases abruptly. With that abrupt increase in thickness is an equally abrupt change in the amount of volatile. It seems probable that the anthracite of Pennsylvania is due to the long continuance of coal-making periods during which the chemical change was unchecked, leading eventually to complete loss of the hydrogen and oxygen.

At the conclusion of the paper, discussion followed, but failed to shake the speaker's main points. A paper by J. E. Wortman, on 'The Geology of the Bad Lands,' was postponed until the next meeting.

J. F. KEMP,
Secretary.

SCIENTIFIC JOURNALS.

BULLETIN OF THE AMERICAN MATHEMATICAL SOCIETY, MARCH.

Arthur Cayley: PROFESSOR CHARLOTTE ANGAS SCOTT.

The Theory of Functions: PROFESSOR W. F. OSGOOD.

On the Introduction of the Notion of Hyperbolic Functions: PROFESSOR M. W. HASKELL.
Notes; New Publications.

THE JOURNAL OF THE AMERICAN CHEMICAL SOCIETY, APRIL.

The Superiority of Barium Hydroxide Solution as an Absorbent in Carbon Determinations in Steel: JAMES O. HANDY.

The Contributions of Chemistry to the Methods of Preventing and Extinguishing Conflagration: THOMAS H. NORTON.

Note on the Estimation of Iron and Alumina in Phosphates: K. P. McELROY.

Some Practical Points in the Manufacture of Nitroglycerol: J. E. BLOMÉN.

Methods for the Examination of Glycerol for use in the Nitroglycerol Manufacture: G. E. BARTON.

Estimation of Tellurium in Copper Bullion: CABELL WHITEHEAD.

The Use of Sulphurous Acid ($HNaSO_3$) in Manufacture of Glucose Syrup and Grape-Sugar: HORACE E. HORTON.

The Furfurol-Yielding Constituents of Plants: C. F. CROSS, E. J. BEVAN and C. BEADLE.

The Separation of Solid and Liquid Fatty Acids: E. TWITCHELL.

Improved Methods of Water Analysis: IRVING A. BACHMAN.

A Cheap Form of Self-Regulating Gas Generator: W. W. ANDREWS.

Some of the Properties of Calcium Carbide: F. P. VENABLE and THOMAS CLARKE.

Note on the Determination of Zinc: P. W. SHIMER.

On the Determination of Cane-Sugar in the Presence of Commercial Glucose: H. A. WEBER and WILLIAM MCPHERSON.

On the Action of Acetic and Hydrochloric Acids on Sucrose: H. A. WEBER and WILLIAM MCPHERSON.

Method of Determining Chromium in Chrome Ore: EDMUND CLARK.

New Books; Notes.

NEW BOOKS.

Manual of Geology. JAMES D. DANA. Fourth Edition. New York, American Book Co. 1895. Pp. 1087.

A Course of Elementary Practical Bacteriology. A. A. KANTHACK and J. H. DRYSDALE. London and New York, Macmillan & Co. 1895. Pp. xxii+181. \$1.10.

Elementary Biology. EMANUEL R. BOYER. Boston, D. C. Heath & Co. Pp. xxi + 235.

The Geological and Natural History Survey of Minnesota. N. H. WINCHELL. Minneapolis, Harrison & Smith. 1895. Pp. 254.